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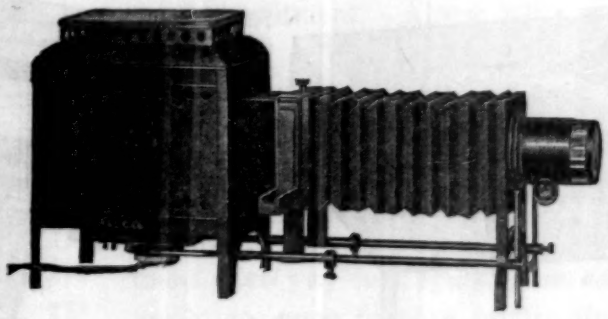

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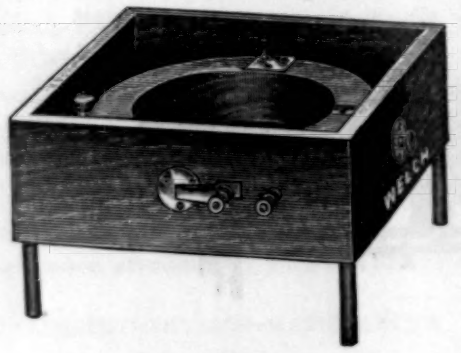

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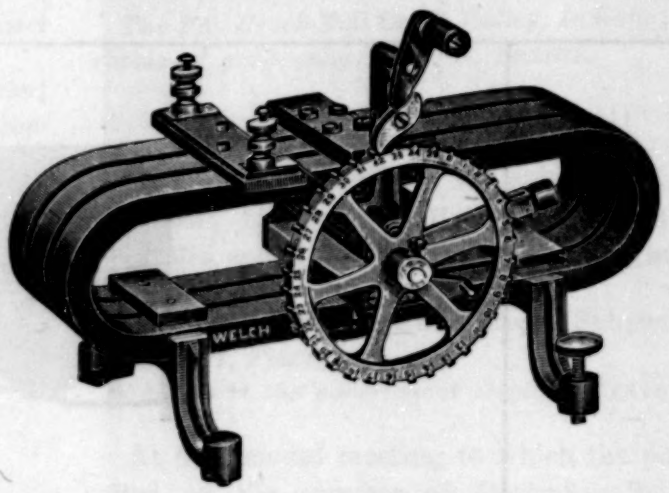

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PROBLEMS OF HUMAN VARIABILITY[1]

WHEN I was first introduced to the literature of experimental psychology, there was one characteristic, frequently recurring phase of the numerical expression of experimental results that commonly aroused a mild resentment. Along with central values, which were then almost universally expressed in terms of the average, there was usually a smaller numeral which bore the curious legend of "mean variation." As a student I had a suspicion that the legend had some connotation of disapproval and regret. In later laboratory experience, of course, that early resentment gave way to a complacent tolerance. The mean variation came to be a legitimate part of the game. But I never outgrew the suspicion of disapproval and regret. As the years have passed, this suspicion has grown into a conviction that contentment in the mass expression of human variations is not good science.

As my scientific interest developed into experimental investigations of my own, variability loomed more insistently and menacingly as a great barrier to real science. Notwithstanding all practicable care to preserve the constancy of stimuli, and notwithstanding the increasing reliability of recording techniques, the experimental shadow was never lost. It was only slightly reduced. Every effort to state the effects of experimental investigation in exact terms sooner or later encounters the same anomalous limitation. If, peradventure, in consequence of a great number of measurements and their statistical treatment, a point is reached where further data have relatively little effect on the central value, the fact of variability still remains to invalidate the application of that abstract central value to the next actual facts of experience. Apparently variability is quite as real as the central value. Notwithstanding our accumulated fund of painstaking measurements, there are conspicuously few dependable constants in psychology. For a science that seeks to express itself in terms of invariants the facts of mental life are woefully unaccommodating.

Two scientific experiences strengthened my conviction that variations in psychology must be taken seriously. The first was technical. In the study of fatigue, as well as in the study of the neuro-muscular effects of alcohol, the most conspicuous feature of the

[1] Address of the vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Cincinnati, December, 1923.

relatively exact records was their variability. The surprising thing to me was that any central tendency at all was discoverable. No two successive eye-movements took the same time, followed the same path, or had the same beginning or end. No two successive lid reflexes, knee-jerks, word reactions or thresholds gave the same measurements. Identical records apparently did not occur. It seemed to me that if the experimental modification of neuro-muscular processes were to develop, some one must undertake the arduous task of studying the conditions and range of their normal variability.

The other experience was more theoretical. It developed under the dual influence of Sherrington and Verworn. Out of it came the belief that neuro-muscular variations were not artifacts to be statistically lumped and treated as though they were errors of measurement, but that they represented realities which might well be quite as significant as the conscious aims of measurement in which they occurred.

The practical importance of variability needs only to be mentioned. The capacity to learn by experience is fundamental to the healthy human mind. It is a measure of intelligence. Stereotypy in thought or action is a symptom of abnormality. Complete invariability of response is observed in no living organism.

Variability is a factor also in life's tragedies. The operation of a power-press is one of the more monotonous automatized occupations. Close escape of the operator's fingers is common. An extreme in the normal distribution of escapes means disaster. Doubtless all accidents may be reduced to terms of position in the normal distribution of human variations. The automobilist cuts a little too close. The machine worker brings his hand a little too near to the gears. The locomotive engineer thinks of extraneous affairs a little too absorbedly. If accidents were modal or close to the mode, working conditions would be intolerable. The prevention of accidents as an ideal requires a system of checks and safeguards that raises the entire distribution area of variations above the limits of safety. This seems fundamental to the theory of accident prevention.

Human variability has been regarded scientifically in three typically different ways. Historically, the first, and probably still the most widespread reaction of the scientific consciousness to the variability of human nature, is doubt as to the possibility of a science of the human mind. If I remember rightly, Kant held against the possibility of a science of psychology because of the impossibility of reducing mental phenomena to mathematical formulae. A consciousness of difficulty, however, like a consciousness of ignorance, is more apt to stimulate scientific endeavor than to paralyze it. Herbart, Fechner, Donders, Cattell, Ebbinghaus, Spearman, Thorndike and the long line of those who have sought numerical expression for mental events form an interesting and suggestive sequel to Kant's dictum. But there are still real differences of scientific opinion as to whether psychology may properly be called a science in anything but aim.

A second reaction to human variations has been to regard them as accidents and to express them in terms of the theory of chance error. This is the reaction in which psychology proceeds by analogy with the physical sciences. There is, however, an important difference between the statistical elaboration of a group of physical measurements and the elaboration of variations in a psychological experiment. In the former it is assumed that there is only one real value and that this reality, or an indefinitely close approximation to it, together with a statement of the probable error of the approximation, can be determined by the statistical treatment of the data of numerous more or less imperfect measurements. In the latter case—that is to say, in psychological experiments—even when one arranges for records of the highest technical reliability, the variations are not eliminated. They are, then, not mere accidents of measurement, but are inherent in the facts which are under investigation. The term "probable error" under such circumstances is an anomaly.

While the emphasis on unsystematized variability as such tends to skepticism or agnosticism with respect to the possibility of a science of mind, the search for mental invariants to be statistically elaborated from the data of experiment leads to artifacts. While some students of the exact sciences are still frankly skeptical with respect to the scientific character of psychology, those whose need of a knowledge of human nature is great, as the psychiatrist, the educationist and the personnel manager, point out that most psychological generalizations are relatively barren in the treatment of individual cases, or when applied to the complexities of actual life as they find it. There would seem to be possible a clear way out of the dilemma in a third attitude—namely, in the systematic exploration of the anomalies that are commonly grouped in some statistical measure of variability. Analogy from the psychology of individual differences would seem to justify the expectation that real variations are just as significant as uniformity, and that the investigation of human variations is an investigation of reality.

Much of the recent service of psychology in education, in mental disease and in personnel has been due to the readjustment of its scientific aims to the peculiarities of the materials with which it deals. Treating each individual as a special combination of capacities, experiences and accomplishments has been found

to be vastly more efficient than treating individuals as though they were all alike. This service of individual psychology seems to have only just begun. It is reaching almost daily into wider fields of science and practice. But it is still handicapped by recurring popular demands for a system of stable invariants, such as mental age, experience, habits, instincts, sensations, memory types, emotions and the like.

Differences in the attitude toward human variability has resulted in some confusing differences of emphasis in the scientific accounts of some of the best known facts of our mental life. When the repeated stimulation is met successively by similar reactions, the product of the search for relatively stable objects leads to the doctrine of habit. In the traditional accounts of habit, variations are commonly ignored. When noted, they are treated as disturbances of the real process, as relatively insignificant accidents in the great tendency to human uniformity.

As every one knows, the facts of human nature are never so simple as the schemata of habit imply. Probably no two repetitions of a human reaction would ever prove to be identical, if the records were complete and the units of measurement were fine enough. The stabilization of long training greatly limits variability, but even in the best-trained performances, like a musician's execution of classical music, no two instances seem to be exactly alike. Within the limits of virtuosity there are apparently instances approaching genius and moments approaching mediocrity, a best performance and a poorest one, probably never two that are identical. In less thoroughly standardized reactions there is an indefinite variety of transfer, substitution, elision and addition, a continuous evolution of new patterns in which the old patterns are partial conditions rather than archetypes.

On the other side of the methodological dilemma, the traditional doctrine of learning emphasizes the variations of reaction to a succession of similar stimuli. Quantitative investigations of learning show that invariant habit is never found in any adequate record of human performance. Habit functions as a limiting concept of learning. It is analogous to the hypothetical final plateau of development, which is seldom, if ever, quite reached in experimental investigation.

Analogously, the traditional scientific accounts of sensation, perception, memory, instinct and reaction time tend to overemphasize the hypothetical invariants, whereas practical needs and practical insight make it plain that invariability in human life is a myth. It is a great service of the modern German school to show that sensations as invariant elements of mental life do not exist and would be valueless if they did exist. We long ago abandoned the hypothesis of mental faculties, but the spirit that postulated them still survives in our traditional account of habits, reflexes, instincts and ingrams, as well as in our efforts to test human capacities.

It is a sign of hope that wherever variability has been investigated, as, for example, in the fatigue curve, the curve of forgetting, the curve of work, the effects of drugs and climate, motivation and the unconscious conditions of consciousness, the results have regularly increased our working knowledge of human action, although at the cost of simplicity of our scientific generalizations. Contrariwise, the treatment of mental facts as relatively fixed constants or systems of objects has led to grave errors in the history of psychology. It has been the tendency which underlies the recrudescence of hypothetical faculties, types, instincts and atomic sensations. The description of mental facts as more or less complexly conditioned integrative processes keeps close to the facts, even if it fails as yet to satisfy our demands for scientific simplicity.

The first great problem of human variability—namely, what to do with it—seems to me to have only one answer that is congruent with a true scientific attitude. As far as they prove to be real we must treat them as real without apology and without regret. The least satisfactory treatment would be to leave them massed in a statistical penumbra.

Doubtless law constitutes the aim and end of all scientific thinking. Nothing that I have in mind is opposed to that generalization. The difference between a science that seeks a relatively stable system of things and their qualities and a science that would investigate variability is not merely that the equations of the latter are more complex. The latter regards the changing complex sum-total of conditions not simply as a changed system, but as a system of changes. The former asks, What are the unanalyzable elements of mental life? What is a given person's reaction, mental age or intelligence quotient? What is his span of attention, etc.? The latter asks, What are the differential consequences of successive stimulation? How is a reaction affected by antecedent and concurrent processes? What are the integrative processes that we call attention, consciousness and personality? This is what some of us mean by dynamic or conditional psychology.

THE GENERAL PHYSIOLOGY OF SENSATION

The moment one adopts a general scientific attitude toward the fundamental problem of human variability a host of special and particular problems assume new significance. Between general physiology and the psychology of sensation there is a great gulf, especially in the conception of the stimulus. Our standard psychologies still commonly state that the stimulus for visual sensation is light. The fact that visual sensation disappears if light falls continuously on any

part of the retina is laid to fatigue or adaptation. The physiological fact seems to be that living tissue, whether it is sense organ or nerve muscle preparation, is excited only by a change in its external vital conditions. Light is not the stimulus. It is change in illumination that excites reaction. While the sense organs seem to be peculiarly differentiated for long-continued reactions to single changes, a constant sensation from constant energy is never discoverable in any concrete instance. The normal invisibility of the retinal blood-vessels illustrates the importance of change in the concept of the stimulus. The eye-movements during continuous fixation are almost equally suggestive. A law that correlates energy change with a uniform sensory intensity would be of equal theoretical importance to the Weber-Fechner law.

The general physiology of sensation has yet to be written. It never will be as long as we cling to the delusion that sensations are stable entities of the mental life. I believe that we need a thorough-going revision of the tradition of sensation from the standpoint of the general theory of irritability.

THE PROBLEM OF REPEATED STIMULATION

A second group of problems of human variability of scarcely less fundamental importance concerns the effects of the repetition of stimuli. When the patellar tendon is struck twice in rapid succession, a curious phenomenon is observed. If the muscle responses are measured accurately in terms of muscle thickening, the second response equals the first only very rarely. We know little enough about the facts, still less about the reasons. Briefly, it appears that if the second knee-jerk stimulus follows the first within half a second, it finds the reflex arc in a condition of decreased irritability, called the relative refractory phase.

First discovered in the heart muscle, refractory phase appears to be a universal phenomenon of irritable tissue. Its characteristic duration and some of its modifications have been shown in nerve and in some of the reflexes.

Several years ago, with the support of the Ernest Kempton Adams Fellowship, I undertook an exploration of some of the elementary conditions of human variability. Neither the considerations that influenced the selection of processes, nor the methods of stimulating and recording them need now detain us. Some of the results, however, are directly relevant to our present discussion.

In all the processes which were measured there appeared more or less clear analogues of the relative refractory phase of the knee-jerk. These analogues are of especial theoretical interest in the unsystematized and systematized cortical reactions. In the latter they play an amazing rôle.

REFRACTORY PHASE OF VOLUNTARY REACTIONS

There is no standard technique for investigating the refractory phase of a cortical reaction. There is no general agreement that such a phenomenon exists. On the contrary, one might suppose that there is no limit to the frequency of repetition of voluntary reactions. Theoretically, however, it is highly improbable that refractory phase is an exclusive phenomenon of the lower neural arcs. The evidence from general physiology would be entirely opposed to such a conjecture. Refractory phase seems to be a universal phenomenon of sensitive tissue.

I believe that something very much like refractoriness regularly appears in cortical reactions in spite of appearances to the contrary. Take a hypothetical case for illustration. If several visual stimuli were to follow each other at a fixed interval of 0.5″, reactions to the recurring stimuli undoubtedly would not be retarded or diminished by the fact of the sequence. On the contrary, the latency of reaction would tend to diminish to a vanishing point. It should be noted in this hypothetical case, however, that the rhythmic succession of identical stimuli is not merely a repetition of the first stimulus. It becomes part of a total stimulus situation which is responded to as a whole by a systematized series of acts.

In such a case, a reaction elicited by the systematization before its natural stimulus has been given will not be revoked with a normal reaction latency by its natural stimulus. When the normal stimulus appeared, it would not be reacted to at all. It would fall within the relative refractory phase of the anticipatory reaction. So anticipatory reactions often displace normal responses and confuse or inhibit them.

This tendency of an anticipatory systematized reaction to inhibit natural reactions is well illustrated in the pursuit reactions of the eye to a moving pendulum. When the pendulum begins to move, there is a single reaction latency of approximately 0.2″. Thereafter there is no observable reaction latency. A normal person picks up the rhythm of the pendulum swing and follows it with adequate ocular pursuit movements until fatigue sets in. That is to say, the successive swings of the pendulum are not reacted to as isolated events at all. Anticipatory reactions to the rhythm of the pendulum inhibit the discrete reactions with normal latency.

There is some evidence that a similar relative refractory phase exists not only in experimental reactions, but throughout normal mental life. If there were no relative refractory phase of the cortical processes, the last experience, whether objectively or subjectively conditioned, should theoretically tend to reiterate itself in endless repetition, on the basis of recency. That is to say, without something like a refractory phase, an idea or memory image would main-

tain itself endlessly in consciousness until some stronger stimulus eliminated it. This is directly contrary to the fact. Only occasionally does a mental image persist so tenaciously that one can think of nothing else. Such instances are conspicuously rare in normal mental life and are usually conditioned by emotional reinforcement.

Somewhat more often, motor acts take on the characteristics of stereotypy. There seems to be real difference between motor and sensory refractoriness. Witness the difference in the effects of continuous rewhistling of the same melody on the whistler and his auditors. A good story or joke is perennially good to the one who tells it, but it soon becomes intolerable to the one who is forced to listen to its repetitions.

Art differs from other forms of experience in the brevity of its relative refractory phase and our readiness for repetition. A joke is stale on repetition. A great work of art is enjoyed over and over again. This may be due to emotional reinforcement, though we do not yet know how pleasure could operate to diminish refractory phase. We do not ordinarily care to read the same novel twice. We tend to avoid the immediate repetition of a walk or any other experience. This antipathy to immediate repetition shows itself in the craving for diversity of experience, for change in work, in dress, in recreation. One's appetites and desires follow the same general scheme. The recency of an experience acts to strengthen the disposition for its return, but it also operates to inhibit its immediate repetition. Any calamity may be preferred to being bored by endless repetition.

Our difficulty in experimentally demonstrating the existence of a refractory phase in voluntary reaction we have already mentioned. It appears to be impossible to apprehend a succession of discrete identical stimuli as disconnected units. They naturally and inevitably fall into more or less complicated rhythms, groups and totals. Rhythm, generalization and even science itself express this tendency. It is certain that our minds refuse to keep separate a mass of isolated repetitions. It is fairly clear that the repetition of a word stimulus is not a simple repetition. The second instance differs from the first in that it tends to form a series with the first instance. Apparently the impossibility of reacting to a series of identical stimuli as though they were isolated events manifests itself in system building and in systematic memory. We have experimental evidence of this function both in our word reactions and in the memory experiments.

By a peculiarity of our exposure apparatus in the word reactions an exposure once made was permanent until the experimenter reset the exposure mechanism. This circumstance will doubtless be criticized by many of those who are learned in tachistoscopic procedure. Protracted exposure has seldom been used in reaction experiments. This is not the place for an elaborate defence of our technique. One may say in passing that it corresponds more nearly to normal reading than very short exposures. From the standpoint of our present discussion the significant fact is that notwithstanding the long exposure the word was regularly spoken but once. There was abundant opportunity to repeat it many times. Why was it said but once?

Analogous inhibition of repetition occurs in normal reading. The just fixated word does not disappear immediately after one has read it. On the contrary, it remains visible in the peripheral visual field for some time, but it is seldom read a second time and practically never reread without definite intention and refixation. Just as it remains more or less indistinctly visible in the visual field, it also remains more or less indistinct in the fringe of consciousness. Reading requires a certain persistence in consciousness of what one has just read. Residual effects of this sort may last some time.

Both in our experiments and in normal reading the tendency against repetition may be regarded as a consequence of the systematic connections in which the stimuli appear. It would be entirely possible to arrange for a different kind of reaction in which the word was repeated "n" times after exposure. Such a complicated reaction must, however, not be confused with the repetition of reactions. When a complicated reaction of this sort was completed (after "n" acts), it would not naturally be repeated except in response to a special demand.

It is in consequence of the general disinclination to repeat reactions that words and phrases are not commonly reiterated in close juxtaposition in good writing. To do so is not conducive to fluent reading. A mechanism for protection against useless repetitions is not difficult to imagine. The circumstances clearly require some agency that operates on the analogy of a relative refractory phase. If it is not a relative refractory phase, one would have to postulate some other mechanism with identical characteristics and functions.

Refractory Phase in the Development of Reaction Systems

The most complex cortical reactions which were included in our program consisted of a group of words in process of systematization. This would be commonly classified as memorization. In the familiar forms of memory experiments, series of letter groups or words are commonly presented one at a time by a revolving drum. When memorization is complete, speech reactions within the series no longer need to follow the original stimuli. The actually effective stimulus is not the visual presentation of the word

which is spoken, but some more or less remotely connected fact of experience. By a process of association or conditioning, whose neural nature is still a matter of conjecture, the reactions become anticipatory. When the reactions are uniformly anticipatory for each member of the series, memorization is commonly held to be complete.

The method of complete learning is time-consuming. Moreover, it usually fails to show the variations in reaction which were the main problem of our experiments and which we hoped would throw some light on the nature of the process itself. Satisfactory technique consequently must give some measure of decreasing reaction latency as learning progresses. An adequate measure of changes in the reaction latency incidental to learning would obviate the necessity for complete learning.

Our method of eliciting and recording these reactions to word series was analogous to the traditional method of paired associates. In our experiments, however, the measured associations were continuous instead of being merely paired. This made the method a kind of multiple prompting. In brief, it consisted of the following essentials: series of twelve four-letter substantives were exposed letter by letter, beginning with the last letter of the last word and proceeding backward to the first letter of the first word. Reaction to each word as it was thus exposed backwards was recorded by means of a voice-key and an electric marker on lines parallel to the series of words. During the first presentation of any series of words adequate reactions could only occur after the presentation of each word was complete or sufficiently complete for identification. In subsequent presentations of the same series, identification and consequent reaction occurred progressively earlier. After sufficient repetitions, each word was spoken not only before its presentation was complete, but before it began. Such anticipatory reactions could occur only on the basis of some systematizing process. Decrease in the reaction time thus became a measure of the memory factor in the reaction. When a series was completely memorized, all reactions were anticipatory. That is to say, each word was spoken before any part of it was exposed.

In their bearing on our main problem these experiments are of peculiar significance. They constitute measurements of a complete cortical resystematization. It is conceivable that the variability of cortical reactions follows quite a different course from that of the lumbar reflexes. Just how much the two really differ was part of our main inquiry.

Throughout the experimental series paired stimuli were presented for both knee-jerk and lid-reflex. Notwithstanding hundreds of repetitions of these paired stimuli the second stimulus was never anticipated. There was no shortening of the reflex latency. On the contrary, the only change in reflex latency seemed to be in the direction of increasing it. This was true both for the knee-jerk and the lid-reflex. A definite experimental effort to develop a reconstruction of the reflex arc in the direction of a crossed knee-jerk was a complete failure. In spite of hundreds of cases of simultaneous stimulation true crossed reflexes were never elicitated. Similarly, simultaneous stimulation of the right and the left final common paths to the quadriceps by voluntary effort and by normal reflex stimulation, respectively, also failed to produce a reconfiguration of the mechanism of the knee-jerk. The lid-reflex was somewhat less refractory to experimental reconditioning. It was possible to produce a lid-reflex of an approximately normal latency by a knee-jerk stimulus, after a considerable number of simultaneous excitations. Partial cortical resystematization occurred in the memory experiments after a single repetition of the series.

A condition of cortical refractoriness similar to that of the speech reactions is discoverable in the effects of these memory experiments. It will be remembered that the stimulus for speech reaction was the exposure of a word which was brought suddenly into place by the action of a pendulum stop mechanism. Each word remained exposed for a second or more, or until the operator removed the word in readiness for the next exposure. In spite of the continuous exposure the subjects uniformly spoke the exposed word only once. Similarly, in the memory experiments the exposure was protracted. Reaction occurred progressively at earlier and earlier moments in the exposure process. It is noteworthy that after reaction occurred the previously adequate stimuli no longer functioned as stimuli to reaction. They found the reaction system in a relative refractory phase.

In the memory experiments, as in the word reactions, the refractoriness obviously depended on the particular arrangement of the experiment. The set incident to the task determined both the reaction movement and the refractoriness. It would doubtless have been possible to arrange an experiment in which the subject would have continued to repeat the word as long as any part of it appeared in the field of view. Refractoriness in this case would have been reduced to the relatively short refractoriness of the motor system. The instructions produced a neuropsychological system whose refractoriness was of a peculiar order. The subject did not keep repeating the just exposed word, though the recency of the exposure should theoretically have rendered it eligible for repetition. The experimental task overbalanced the effects of recency. We would reemphasize the presumptive importance of this in practical life. Without it all our lives might be spent in the repeti-

tion of the first chance experience. In other words, refractoriness to repetition appears to be the first condition of resystematization. Where such refractoriness is low, as in echolalia and in stereotypy, learning is at a minimum.

We have already noted evidence in the reflexes that refractoriness to the just previously given stimulus may develop into an increased sensitivity, if the interval between stimuli is carefully adjusted. This zone of increased sensitivity is relatively unexplored, and its relation to the refractory phase is practically unknown. Both are obvious residua of previous stimulation. Increased sensitivity lasts longer than the refractoriness. It may well be that the heightened sensitivity is a positive condition for resystematization, while refractory phase is a negative condition, and that each condition has its own temporal incidence.

THE PROBLEM OF CONCURRENT PROCESSES

The problems of variability in those reactions which are incident to the repetition of identical stimuli and their systematization are enormously complicated in actual life by various concurrent reinforcements, inhibitious and controls. These conditions of variability are practically unexplored.

We are coming to believe in a natural daily rhythm. But its causes, extent and incidence at the various levels of the cerebro-spinal system are unsolved riddles. If there are weekly, seasonal and yearly rhythms, they are even less well understood.

There are better known respiratory and vascular rhythms, which complicate all threshold experiments and limit the effective delicacy of recording devices of the motor processes. However influential these complications of reaction may be, I conjecture that the most important theoretical implications of our experimental attack on the problem of variability concern those variations that are produced by the interaction of the neural delay paths in the higher neural levels.

One of the surprises of this experimental series was the discovery of an arbitrary depression of the knee-jerk by the voluntary depression of the motor system of the thigh. The hypothesis that this was due to a decreased tonus has utterly failed of verification by the most delicate technique that I could devise. One can not arbitrarily dismiss the hypothesis of a centrally aroused decrease of irritability of the reflex arc. The opposite is undoubtedly true. An increase in the irritability of a reflex arc may be centrally conditioned. Voluntary reinforcement of the knee-jerk may produce every possible degree of quadriceps thickening. Contrary to expectation again, this reinforcement may appear as a smooth contraction without noticeable break between the reflex and the voluntary phase.

Controlled reinforcement occurs in the reflex ocular compensation to rotation when the eyes are open. Records from closed eyes show that the reflex compensation may occasionally be entirely adequate—that is to say, it may adequately compensate for the precise angle velocity of rotation. Usually, however, the reflex compensation is quite inadequate until it is controlled by the more accurate visual data of rotation. In case the vestibular and visual data are contradictory, the latter eventually win the competition and control the final common path.

Analogous transfer of the control of the eye-movements from primitive to fine sensori-motor systems occurs in coordinate compensatory eye-movements.

The data are sufficiently clear and sufficiently numerous to permit generalization. In the muscle reflexes and in reflex ocular compensations, reflexes merely initiate motor responses. They have the practical advantages of a low latency response in a presumptively useful direction. That initial response may be and usually is quite inadequate to meet the situation which aroused it. The subsequent fate of the reflex act, its control, reinforcement, inhibition or reversal depends on higher systematizations and the elaboration of data that do not enter into the reflex.

In the case of the knee-jerk this scheme provides for a reasonable understanding of an apparently imbecilic reflex. Any sudden stretching of the muscle such as would occur if one unexpectedly stepped off a low platform evokes reflex contraction within 40 sigma. Whether that reflex shall be sustained, inhibited or reversed depends on the slower cortical elaboration of the available data.

The picture is not merely a pretty mechanism of great flexibility and efficiency. It points towards a paradigm of all instinctive action. The reflexes represent the simplest and most mechanically persistent of instinctive acts. If they regularly meet rivalry, competition, suppression and reinforcement from superior systems, the more fluid instincts might still more reasonably be expected to meet analogous modifying circumstances and controls. It looks as though the reflexes and other instincts might well reduce to primitive initial responses in predetermined directions. I doubt if their supposititious driving power is more or less than that. In the simpler and more mechanical instinctive acts there is clear evidence that higher centers regularly assume control of the initiated response without disaster, pathological implication or any other disturbances except a moment of systemic rivalry and competition.

The data which I have laid before you are necessarily very limited. I can not hope that all the details are convincing, but I shall be disappointed if we can not all agree on the fundamental thesis that human variations are worth rescuing from the scrap

heap of mass statistics by concerted and systematic attack.

I hope, furthermore, that most of us would accept the related proposition that the search for integrative processes is quite as promising as the search for mental elements, thresholds, reflexes, instincts, complexes or any invariant artifact whatsoever, whether introspective or behavioristic.

RAYMOND DODGE
WESLEYAN UNIVERSITY

JOHN MAXSON STILLMAN, 1852–1923

JOHN MAXSON STILLMAN, professor of chemistry and vice-president emeritus of Stanford University, was born in New York City on April 14, 1852, the son of Dr. Jacob Davis Babcock Stillman and Caroline Maxson (Stillman). He was graduated from the University of California in 1874, and received the degree of Ph.D. from his Alma Mater in 1885. A student in chemistry in Strassburg and Würzburg, in 1875 and 1876, he returned to this country as instructor in chemistry at the University of California, where he remained until 1882. He then went to Boston as chemist of the Boston and American Sugar Refining Companies until 1891 on the foundation of Leland Stanford Junior University, in which institution he served as professor of chemistry for twenty-six years, becoming vice-president in 1913, and retiring under the age limit as emeritus in 1917. He was the author of numerous articles on chemical matters, covering especially the organization of certain vegetable compounds, the ammonia compounds of inorganic chlorides, the molecular lowering of the freezing point in diphenylamin and naphthylamin, the precipitation of calcium and magnesium in the purification of water, the poisonous elements in whisky. Most of these impressed him as little worse than ethyl alcohol itself. In his later years he made a specialty of the history of chemistry, an important piece of research being on the life and work of Paracelsus (1921). Lately he completed a volume on the early chemists and alchemists, soon to be published. He was a member of the American Chemical Society, of the American Institute of Chemistry, of the Deutsche Chemische Gesellschaft, and a fellow of the American Association for the Advancement of Science. He died at Stanford University, December 14, 1923.

Such is the condensed academic record of one of the most scholarly of chemists, most devoted of teachers and most lovable of men. As an intimate associate for half a lifetime, I can speak feelingly of his strength and virtue, and of the indebtedness to him of the new university through all its early growing pains.

At the opening of Stanford University on October 1, 1891, Dr. Stillman was one of the fifteen teachers chosen at the modest but ambitious outset, one of the three senior members in a remarkable group, who remained with it for a generation. Without invidious comparison, I may note these members of this first faculty who have stood steadily in the first rank in respect to scholarly attainments, productive work, educational wisdom and friendly helpfulness. These were the late John Caspar Branner, professor of geology, first vice-president and second president; John Maxson Stillman, professor of chemistry, the next vice-president; and Charles David Marx, professor of civil engineering, vice-president after Dr. Stillman's retirement as Emeritus, in 1917. All three were unusually capable in each of the respects I have enumerated, but Dr. Stillman's especial virtue lay in the line of wisdom. No better faculty man ever helped a university and the need of sound judgment and wise administration was never greater than in the six lean years ("the long fight") which followed the death of the founder (1893 to 1899), when the entire prospective endowment was tied up by wanton litigation. There is an Albanian proverb, "Open a cask of sugar and flies will come all the way from Bagdad." The most insistent of these litigants was the United States Government itself, which claimed the entire endowment in view of the indebtedness of the Central Pacific Railway, not then due and which was paid in full with interest as soon as its bonds had matured. The government was three times non-suited in Federal Courts—at last in the Supreme Court, the university meanwhile living from hand to mouth under conditions of supreme difficulty.

It may interest the thousands of Dr. Stillman's friends and students to know that of all the professors at Stanford he was the only one in any degree selected by the founder himself. Governor Stanford said to me that his old friend, Dr. J. D. B. Stillman, has left a son who had been a teacher in the University of California and was then a professional chemist, living at Brookline, Massachusetts. With this hint, I visited Boston to see Dr. Stillman, and being thoroughly pleased, I offered him the chair of chemistry. This he as promptly accepted, declining to consider an advance in salary for his company, on the ground, as he said, that "it would only tend to confuse his mind." We thus secured (as I have elsewhere stated) "one of the wisest teachers I have ever known and one of the most thoroughly beloved. His dear wife (Emma Rudolph Stillman), I may add has ably seconded him in every relation, and few other Stanford homes have contributed as much as theirs to the social well-being of the community." (Days of a Man, I, 398).

DAVID STARR JORDAN

SCIENTIFIC EVENTS

NEW KEEPERS AT THE BRITISH MUSEUM[1]

IN view of the approaching retirement of Sir Ernest Budge, and Dr. A. Smith Woodward from their posts at the British Museum, the principal trustees have made the following appointments, to take effect from the dates of their retirement, respectively:

Dr. H. R. Hall, M.B.E., D.Litt., to be keeper of Egyptian and Assyrian antiquities.

Dr. F. A. Bather, F.R.S., D.Sc., to be keeper of geology.

Sir Ernest Budge will retire on April 9 and Dr. Smith Woodward on May 23.

Sir Ernest Budge, who will be 67 next July, was made keeper in 1893 when he was only 36. He has produced an enormous number of books on the languages and histories of the ancient nations of the nearer east, some of them the results of excavations at Nineveh, and in Mesopotamia, Egypt and the Sudan. His knowledge of Assyriology and Egyptology is the more remarkable as he was self-taught. A man of original personality and extraordinary energy and tenacity of purpose, he will be greatly missed at the museum, where he has made important additions to the national collection.

Dr. H. R. Hall, who succeeds Sir Ernest Budge as keeper, is the son of the late Mr. Sydney Hall, the painter. From Merchant Taylor's School he went up to St. John's, Oxford, and in 1896, being then 23, entered the Department of Egyptian and Assyrian Antiquities. He has excavated for the Egypt Exploration Fund, and directed the British Museum excavations in South Babylonia (Ur of the Chaldees, Tell el-Obeid, and Abu Shahrein), and has done other important work, notably with Mr. L. W. King. During the war he rendered valuable political service in Mesopotamia.

Dr. Arthur Smith Woodward, who will be 60 on May 23, entered the museum in 1882. He has been particularly concerned with extinct vertebrata, especially of South America. To the public he is perhaps best known for his work on the discovery and interpretation of the Piltdown skull. Dr. Woodward has taken an active part in congresses of geologists and paleontologists, and his contributions to scientific literature have been important. He has received medals and other honors from learned societies, including the Prix Cuvier from the Institut de France in 1918.

Dr. F. A. Bather has been deputy keeper of the Department of Geology since 1902. From Winchester he went up to New College, Oxford, and obtained the Rolleston Prize for research in biology. In 1887 he entered the museum, and he has established his reputation as a geologist, and also as an expert on museum technique.

[1] From the London *Times*.

THE SPRING MEETING OF THE AMERICAN ELECTROCHEMICAL SOCIETY

THE spring meeting of the American Electrochemical Society in Philadelphia on April 24, 25 and 26, promises to be one of interest to electrochemists. It would be difficult to select a more suitable city for a place of meeting than Philadelphia. Located amid beautiful surroundings and adjacent to enormous industrial establishments, it offers many features of personal appeal and interest, in addition to many interesting excursion trips.

The headquarters will be the Bellevue-Stratford Hotel. The technical meeting will be devoted to a symposium on "Organic Electrochemistry." Dr. C. J. Thatcher, chemical engineer, of New York City, will act as chairman. The discussion will cover the applications of electrochemistry in the preparation of organic chemicals, both in this country and abroad, and in particular, the future possibilities for the commercial production of such compounds.

The other symposium covers "Recent Progress in Electrodeposition." Mr. S. Skowronski, research chemist of the Raritan Copper Co., is chairman. A series of papers have been planned, covering "Electro-Refining of Metals," "Electro-Reduction of Metals," "Electro-Forming of Metals" and "Electro-Plating of Metals."

A large number of papers have been promised for the Electro-deposition symposium. There have been many important changes taking place within the last ten years in electrolytic refining of metals and electrolytic winning of metals from leaching solutions and we hope that at the Philadelphia meeting many of the innovations will be brought up for discussion, and a large attendance is expected at the symposium.

Another attraction of the technical program will be a round table discussion on "Electric Furnace Refractories." In addition to the round table discussion, there will be several interesting papers presented dealing with various phases of the subject.

In addition, two lectures, one by Dr. D. J. Barnett, at the Franklin Institute, the other by Mr. John Mills, will be delivered.

To those interested in excursion trips to industrial plants and places of interest, Philadelphia offers many attractions. Trips have already been arranged to visit the Philadelphia Mint, Disston Company, Dodge Steel Casting Company, Ajax Wyatt Company, Westinghouse Electric and Manufacturing Company and an Airplane factory. Arrangements have also been made which will afford an opportunity to society members to visit Edgewood Arsenal on Saturday, April 26. The arsenal consists of eight main

divisions: administrative, chemical, mechanical, medical, research, troops, plants and production, property and chemical warfare school.

Dr. Arthur E. Gibbs, of the Pennsylvania Salt Manufacturing Company, is chairman of the local entertainment committee. One of the several entertainment features will be a boat ride and shad dinner.

THE BANTING RESEARCH FOUNDATION

THE discovery and development of insulin by Dr. F. G. Banting, Mr. C. H. Best and other cooperating investigators has brought relief to a multitude of sufferers from diabetes throughout the world. At a low price this boon has been placed within reach of all. But it is well known that only a beginning has been made in alleviation even of this one malady. Notwithstanding the magnificent advances that have been effected in arresting or averting many of the most grievous attacks of disease on human life, mankind is beset by enemies. Their strategy must be discovered and circumvented. This can be done only by patient research conducted in the main by skilled investigators who devote their lives to scientific enquiry. For these investigators the public at large must provide the means of support, for they it is who benefit immensely thereby. Such work has been going on quietly all over the world. Laboratories in the universities have groups of investigators working in cooperation under the direction of competent scientists. But only now and then does a result such as Dr. Banting achieved strike the imagination of the world. It is therefore but appropriate that advantage should be taken of it to appeal to the grateful public for support in making possible the continuance and prosecution of this work and of other investigations in medical science. To effect this and to signalize the discovery and the development of insulin, the Banting Research Foundation has been created.

The purposes of this foundation have been defined to be:

(a) To provide, in the first instance, further funds for the support of the Banting and Best chair of Medical Research at the University of Toronto.

(b) To establish a fund for the adequate financial support of such scientific workers as may have proposed definite problems of medical research, and for whom funds are not otherwise available. Such assistance may be given to persons working in the University of Toronto or elsewhere.

All financial arrangements in connection with the collection and reception of the principal and subsequent expenditure of the income of the fund have been vested in a board of trustees, the members of which are appointed for a term of three years subject to reappointment at the end of their respective terms of office. Trustees have now been appointed as follows:

Sir Robert A. Falconer, chairman, president of the University of Toronto.

Lieutenant Colonel R W. Leonard, honorary treasurer, member of the Board of Governors of the University of Toronto.

The Reverend Canon H. J. Cody, chairman, Board of Governors, University of Toronto.

C. S. Macdonald, general manager, Confederation Life Association.

W. E. Gallie, surgeon-in-chief, Hospital for Sick Children, Toronto.

Professor J. G. FitzGerald, professor of hygiene and preventive medicine, director of the Connaught Laboratories of the University of Toronto.

Professor V. E. Henderson, professor of pharmacology, University of Toronto.

Mr. John W. Rogers.

Subscriptions to the fund will be welcome at any time and should be made payable to the Banting Research Foundation, Toronto, Canada.

F. LORNE HUTCHISON,
Honorary Secretary

PRESIDENT ELIOT'S NINETIETH BIRTHDAY

UNDER the auspices of the Harvard Alumni Association, the Associated Harvard Clubs and a Committee of Citizens, of which President Coolidge was chairman, a meeting was held in Sanders Theater on Thursday, March 20, to celebrate the ninetieth birthday of Charles W. Eliot, president emeritus of Harvard University.

The program of addresses included greetings from President Lowell on behalf of the Corporation; George Wigglesworth, '74, on behalf of the Board of Overseers; Dean L. B. R. Briggs on behalf of the faculties of the university; Charles T. Greve, '84, on behalf of the Asosciated Harvard Clubs; Charlton MacVeagh, '24, senior class orator, on behalf of the undergraduates; President James R. Angell, of Yale University on behalf of the colleges, universities and learned societies of the United States; Governor Cox on behalf of the Commonwealth of Massachusetts and Chief Justice Taft on behalf of the American public.

Dean Briggs's address included the presentation of congratulatory messages from each of the faculties of the university. Mr. Greve's address included the presentation of congratulatory messages from Harvard Clubs in all parts of the world. President Angell's address included the presentation of congratulatory messages from other colleges, universities and learned societies.

At the conclusion of the congratulatory addresses, President Eliot made a response, after which accompanied by the other speakers, he proceeded to the yard, where, in front of University Hall, he received the greetings of the assembled students of the university.

The American Association for the Advancement of Science, of which Dr. Eliot was president in 1915, sent a congratulatory message, in the following form:

TO CHARLES W. ELIOT

For forty years president of Harvard University, of whose great traditions you are a part, the American Association for the Advancement of Science, once honored by your election to its presidency, sends greetings on the occasion of your ninetieth birthday. One of the verses in the treasure-house of Greek letters warns against calling any man happy before he is dead. You, Sir, while still active in your work, witness in no small measure its fruition and the high esteem in which it is held by all. Each of the twelve thousand members of the American Association for the Advancement of Science, engaged in the fields of science and of education which you have made more fertile for them, would gladly join in expression of respect, of appreciation and of honor to you, man of science, our master in education, leader in American civilization.

SCIENTIFIC NOTES AND NEWS

THE annual general meeting of the American Philosophical Society will be held at Philadelphia on April 24, 25 and 26. On the evening of April 25, Dr. Dayton C. Miller, professor of physics in the Case School of Applied Science, will give a lecture on "Visible sound," previous to a reception in the hall of the historical society of Pennsylvania. The symposium on the morning of April 26 is entitled "Are the various races of man potentially equal?" Those who will speak are: Professor Franz Boas, Columbia University; Dr. Clark Wissler, American Museum of Natural History; Dr. Aleš Hrdlička, of the U. S. National Museum, and Dr. H. U. Hall, University of Pennsylvania.

ARTHUR STANLEY EDDINGTON, F.R.S., Plumian professor of astronomy at the University of Cambridge since 1913, and director of the observatory since 1914, who was recently awarded the gold medal of the Royal Astronomical Society has been awarded the Bruce Gold Medal of the Astronomical Society of the Pacific for 1923. The awards in both cases were based upon his work in relativity and the internal physical constitution of the stars.

PROESSOR MAX PLANCK, of Berlin, has been elected an honorary fellow of the Physical Society of London.

THE Council of the Royal Society has recommended the following fifteen candidates for membership: Dr. Thomas Nelson Annandale, Mr. Joseph Edwin Barnard, Professor James Fairlie Gemmill, Dr. Mervyn Henry Gordon, Professor Percy Groom, Dr. Christopher Kelk Ingold, Professor Percy Fry Kendall, Professor Louis Vessot King, Professor Louis Joel Mordell, Dr. Thomas Slater Price, Professor Chandrashekara Venkata Raman, Professor Leonard James Rogers, Dr. Alexander Russell, Professor Charles Spearman, Mr. Frank Twyman.

THE gold medal of the British Institution of Mining and Metallurgy, as we learn from *Nature*, has been awarded conjointly to Mr. H. W. Gepp and Mr. G. Rigg in recognition of their joint and individual services in the advancement of metallurgical science and practice, with special reference to their achievements in the treatment of complex sulphide ores, and in the development of the electrolytic process for the production of zinc in the Commonwealth of Australia.

DR. J. L. E. DREYER has been elected president of the Royal Astronomical Society. The vice-presidents are Dr. A. C. D. Crommelin, Professor A. S. Eddington, Professor A. Fowler and Mr. E. B. Knobel.

JUSTICE CURTIS DWIGHT WILBUR, of the Supreme Court of California, who has been appointed secretary of the Navy by President Coolidge, is a brother of Dr. J. Lyman Wilbur, president of the University of California and this year president of the American Medical Association.

REAR ADMIRAL ALBERT PARKER NIBLACK, U. S. N., retired, has been elected a director of the International Hydrographic Bureau and is now on his way to the headquarters of that organization in Monaco. Admiral Niblack was the candidate of the United States for a vacancy on the board of directors which consists of three members.

DR. HERBERT N. MCCOY has been appointed a member of the board of trustees and field secretary for the publication of the International Critical Tables of the National Research Council, with the special object of completing the campaign for the necessary funds as rapidly as possible. About seventy-seven thousand dollars are needed in addition to the sum already pledged or paid.

DR. MARSTON T. BOGERT, professor of organic chemistry at Columbia University, has been elected chairman of the committee on chemical research on medicinal substances of the National Research Council.

AT the thirty-fourth annual meeting of the Association of American Medical Colleges in Omaha from February 28 to March 1, the following officers were elected: *President,* Dr. Ray Lyman Wilbur, Stanford University; *Vice-president,* Dr. Hugh Cabot, University of Michigan; *Secretary,* Dr. Fred C. Zapffe, 3431 Lexington Street, Chicago; *Chairman of the executive council,* Dr. David L. Edsall, Harvard University. The next annual meeting will be held in Boston some time in February, 1925.

LAWRENCE W. WALLACE, of Washington, executive secretary of the American Engineering Council, has

been reelected president of the Eyesight Conservation Council of America.

THE American Association of Cereal Chemists, which in 1923 united with the American Society of Milling and Baking Technology, is publishing a new journal, *Cereal Chemistry*. The editorial staff includes Dr. C. H. Bailey, University of Minnesota, editor-in-chief; C. G. Ferrari, managing editor; Dr. C. L. Alsberg, L. A. Fitz, Mary M. Brooke and H. E. Weaver, associate editors. Publication will be bimonthly during the year 1924, the first issue having appeared in January of this year.

WILLIAM GAERTNER, president of The Gaertner Scientific Corporation, 1201 Wrightwood Avenue, Chicago, Illinois, has been awarded the Howard N. Potts gold medal by the Franklin Institute acting through its committee of science and arts "in consideration of his notable achievement as a designer and maker of scientific instruments, materially contributing to the success of the research in physical science."

T. R. HARRISON has assumed charge of the research department of The Brown Instrument Company, Philadelphia. Mr. Harrison was formerly associated with the pyrometry department of the Bureau of Standards and more recently with the Champion Porcelain Company, of Detroit.

DR. B. M. DUGGAR, of Washington University and the Missouri Botanical Garden, is spending a half year at the Johns Hopkins School of Hygiene and Public Health, continuing an investigation of certain filterable viruses.

PRESIDENT WALLACE W. ATWOOD, of Clark University, is visiting Porto Rico where he will deliver an address at the University of Porto Rico, of which C. W. St. John, Clark, 1916, is dean, and Dr. Frederick Morse Cutler, who took his Ph.D. at Clark, is professor of history.

MISS E. A. MC. GAMBLE, professor of psychology at Wellesley College, is on leave of absence for the second semester, making a tour of European laboratories of psychology and psychiatrical clinics. She will represent the college on the occasion of the celebration at the University of Königsberg of the two hundredth anniversary of Emmanuel Kant's birth.

DR. JOHN H. KYLIN, formerly professor of botany at the University of Lund, Sweden, will give a course this summer on Marine Algae at the Puget Sound Biological Station of the University of Washington.

DR. J. E. SPURR, editor of *The Engineering and Mining Journal-Press*, of New York, gave a series of three lectures on Ore Magmas before the geological department of the University of Wisconsin from March 5 to 7. During the same week there was a conference at Madison on Lake Superior geology, participated in by the members of the Geological Department of the University of Wisconsin, representatives of the Michigan and Minnesota geological surveys and mining geologists from various parts of Lake Superior.

THE program for the meeting of the Philosophical Society of Washington, on March 22, will be as follows: "Electrically Controlled Micrometers," Dr. Charles Moon; "The Development of the Electric Telemeter," Orville S. Peters.

PROFESSOR ALBERT PERRY BRIGHAM, of Colgate University, spent the greater part of February and March meeting lecture engagements in England. He gave three lectures on the "United States, regional and national," at the University of London; four on "Major features of the United States," at Oxford University, and lectured before the Royal Geographical Society, the College at Cheltenham and the University College at Reading.

M. LE DUC DE BROGLIE will deliver the Guthrie lecture of the Physical Society of London on March 20, as part of the society's jubilee celebrations.

PROFESSOR C. G. BARKLA, professor of physics at Kings College, London, will deliver the seventh Silvanus Thompson Memorial Lecture of the Röntgen Society at the Institution of Electrical Engineers, London, April 1.

PROFESSOR A. V. HILL delivered the third Joule Memorial lecture of the Manchester Literary and Philosophical Society on March 4. His subject was "Thermodynamics in physiology."

THE death is announced of John G. D. Mack, state engineering of Wisconsin and former professor of mechanical engineering in the University College of Engineering.

ATTENTION is called in *Nature* to the circumstance that on March 5 occurred the tercentenary of the birth of John Collins, who wrote on merchants' accounts and also on quadrants, dialling and navigation. Widely known in his day, he was admitted a fellow of the Royal Society in 1667, which then met at Gresham College. He was an indefatigable correspondent. After his death, letters were found among his papers from Newton, Gregory, Wallis, Flamstead, Slusius, Leibnitz and others, and in 1712 the Royal Society ordered the publication of some of them. He died November 10, 1683.

WE learn from the *Journal* of the American Medical Association that the American Pasteur exhibit prepared by the Department of Agriculture and dis-

played at the American Museum of Natural History, New York City, in December, 1922, in honor of the centenary of the birth of Louis Pasteur, has been awarded the *diplome de grande prix* by the Exposition Internationale de Pasteur at Strasbourg. This exhibit illustrated all Pasteur's world-famous experiments in fermentation, as well as his work on anthrax, chicken cholera and rabies. Apparatus exactly like that employed by Pasteur was reproduced by the Bureau of Chemistry and arranged in such orderly sequence that one could follow each successive step of the great discoveries. John S. Buckley, of the pathological division of the Bureau of Animal Industry, furnished the material showing the experiments on rabies, etc. The exhibit of the Bureau of Chemistry has been returned to Washington and reassembled in the east gallery of the old National Museum.

A BEQUEST to the Royal College of Physicians of Edinburgh by the late Dr. John Parkins is a prize for the best essay on certain medical subjects. The subject of the essay for the present period, to use the terms of the deed, is "On the effects of volcanic action in the production of epidemic diseases in the animal and in the vegetable creation, and in the production of hurricanes and abnormal atmospherical vicissitudes." The prize is open to competitors of all nations, and its value is about $450.

A CORRESPONDENT writes that the Leningrad Academy of Science is preparing to dispatch 60,000 copies of its publications to scientific institutions in Europe and America.

A CORRESPONDENT writes: "W. F. Taylor, professor of bacteriology, Wake Forest College, School of Medicine, assisted by W. L. P. McBride, has recently isolated from the blood of a case diagnosed as typhoid fever a non-agglutinating typhoid like organism, which he has identified as *Bacterium Columbense*. This organism was first described in 1905, later in 1914, by Dr. Aldo Castellani, director of Government Clinics for Tropical Diseases, at Columbo, Ceylon. (*Centralblatt für Bakteriologie, Originale,* 1914). This case shows that *febris columbensis* is a disease not limited to the tropics, but is found also in temperate climates."

A SPECIAL committee of the Engineering Foundation has reported in favor of a non-commercial investigation into the nature and use of paint and varnish. For this purpose the committee recommends that Congress should place at the disposal of the Forest Products Laboratory about $45,000, which is estimated to be equal to one hundredth of 1 per cent. of the value of paint used annually in this country.

GEORGE I. ROCKWOOD, president and treasurer of the Rockwood Sprinkler Co. of Massachusetts, newly elected vice-president of the American Society of Mechanical Engineers, has endowed a gold medal of the American Society of Mechanical Engineers, to be awarded "in those rare cases when an individual has succeeded by the exercise of his genius and character, in powerfully assisting the fortunes of our country or the general engineering progress of the world."

LORD ATHOLSTAN, who has been conducting a crusade in the Montreal *Star* against tuberculosis and the conditions which produce it, has offered to cooperate with the Quebec Government in a campaign against disease. Towards the cost he will make a personal contribution of $500,000. He suggests that measures should be taken similar to those followed in Framingham, Massachusetts. The so-called "Framingham Experiment" was conducted at the instance of the Metropolitan Life Insurance Company, which gave $100,000 for a "community health and tuberculosis demonstration to be conducted by the National Tuberculosis Association of America under the supervision of a special national committee."

THE fifteenth congress of the German Röntgen Society will be held from April 27 to 29 at the Langenbeck-Virchow Haus in Berlin under the presidency of Professor Wintz. The first day will be devoted to diagnosis, the second to the dosimetry of x-rays, radium and light rays, and the third to technique. Further information can be obtained from the secretary, Dr. Graessier, Buerger Hospital, Cologne.

EIGHTY different industrial motion picture films, valued at nearly $200,000, are now in the possession of the Department of the Interior, Bureau of Mines. These motion picture films were produced at small cost to the government, practically the entire cost being borne by the various industries filmed. Each film depicts the various processes employed in the manufacture and marketing of some well-known American product wholly or partly of mineral origin. The films are loaned free by the Department of the Interior to technical societies, commercial organizations, colleges, schools and churches. They are in constant demand. Three pictures have been completed by the Bureau of Mines during the past month. One, "The world's struggle for oil," portrays the history of the oil industry from early Biblical times up to the present day. Another is the "Story of the gasoline motor," which visualizes the complete operation of a gasoline engine. The third picture, "The story of refractories," presents scenes in the manufacture of refractory brick and other refractory products. Applications for these films should be addressed to the Department of the Interior, Bureau of Mines, Pittsburgh, Pa.

THE Italian parliament has passed a law in regard to the preservation of wild birds which will partly

reestablish the good name of Italy in this matter. The London *Times* says: "Complaints have often appeared in the English Press about the wholesale destruction of insectivorous song birds on the Italian hills and mountains. In the autumn months, when visitors are passing through on their way south, an enormous number of birds, mostly chaffinches, red stars, warblers, and the like, are taken for eating and other purposes. They are generally trapped into nets and the so-called *roccoli*—semi-circular pergola-like arrangements of wooden stakes, under a *camouflage* of creepers and bushes. The new law might have been more radical, but it does something to lessen the evil It forbids altogether the vertical nets which are the most destructive on the mountain passes; it prohibits the snares and imposes several limitations and restrictions on the use of the *roccoli* and other similar appliances. It is to be hoped that gradually the scope of the law will be widened and strengthened, but the legislature thinks it advisable to proceed slowly in the matter, as in some of the northern valleys, where there is a poor population, bird catching is one of the ways of getting a living."

UNIVERSITY AND EDUCATIONAL NOTES

BY the will of the late Dr. Plimmon H. Dudley, formerly steel expert of the New York Central Railroad, the chief part of his estate is left to Yale University for the establishment of a professorship "which shall be devoted to research and instruction in connection with the history and development of the science of railway service." "The bequest is intended," wrote Dr. Dudley, "to aid further in the establishment on a permanent basis of 'the Dudley chair, service of railroads,' provided by Lucy B. Dudley, my late wife. . . . It is likewise my wish that Yale University shall accept contributions from other individuals and corporations for the benefit of the aforesaid professorship. I also expressly desire that the research work in which I have been engaged for many years be continued and in particular the work in connection with the development and improvement of designs of rails, roadbeds and cross ties."

DR. AND MRS. ARCHIBALD CHURCH, of Chicago, have given $100,000 towards the endowment and maintenance of a medical library for Northwestern University. Dr. Church is a neurologist and has been a member of the staff of Northwestern University Medical School for thirty-two years.

LADY STRATHCONA has given $120,000 to McGill University to endow the department of zoology.

THE dedication of the new group of medical buildings of the Department of Public Welfare of the State of Illinois and the University of Illinois took place on March 6. Dr. William L. Noble, president of the board of trustees of the University of Illinois, presided. Addresses were given by the president of the American Medical Association, Dr. Ray Lyman Wilbur; Dr. Frederick G. Banting, of the University of Toronto, Canada; Judge Chauncey H. Jenkins and the governor of Illinois.

DR. WILLIAM FRANCIS GAY SWAN, who resigned his position as assistant demonstrator in physics at the University of Sheffield in 1913 to become chief of the physical division of the Department of Terrestrial Magnetism at the Carnegie Institution in Washington and who was appointed professor of physics at the University of Minnesota in 1918, has been appointed professor of physics at Yale University.

DR. JEAN OLIVER, associate professor of pathology at Stanford University Medical School, has been promoted to full professorship.

DR. C. K. INGOLD has been appointed professor of organic chemistry at the University of Leeds as from October 1 next on the retirement of Professor J. B. Cohen.

DISCUSSION AND CORRESPONDENCE

ON CONCENTRATION OF VITAMIN B

EFFORTS to isolate the so-called "Vitamin B" have thus far failed. A successful beginning in this direction was made by Osborne and Wakeman, who separated from yeast a material in the form of a dry powder which possessed much greater potency than the original yeast. Further concentration of the Osborne and Wakeman material had previously been unsuccessful. We have now prepared a material which is from five to ten times more potent than that of Osborne and Wakeman. The material can be obtained either from the Osborne and Wakeman material or directly from dried yeast. It is prepared by precipitation with a solution of barium hydroxide. As a rule, the barium precipitate contains from 30 to 50 per cent. of the total vitamin present in the solution. However, the material obtained from the precipitate is from five to ten times as powerful as the original material.

The present note is prompted by the article of W. Lash Miller published in SCIENCE on February 29, 1924. In this article Miller reports on the use of barium hydroxide for the preparation and separation of the individual components of Wilders' Bios.

The details of our work will soon appear in the *Journal of Biological Chemistry*.

P. A. LEVENE,
B. J. C. VAN DER HOEVEN
THE ROCKEFELLER INSTITUTE
FOR MEDICAL RESEARCH

THE NAME OF THE SPOTTED FEVER TICK

OPINION 78 of the International Commission on Zoological Nomenclature, just issued by the Smithsonian Institution, is so extraordinary that it should not be allowed to pass without comment. The summary states that "the commission is of the opinion that *Dermacentor venustus* dates from Marx in Neumann, 1897, type specimen Collection Marx No. 122 (U. S. National Museum), from *Ovis aries,* Texas." This is not quite the same as the statement on page 13: "*Dermacentor venustus* Marx in Neumann, 1897, belongs to a form," etc. The latter statement may be regarded merely as an indication of the facts; the former, professing to be a summary, commits the commission to the proposition that the publication by Neumann is valid from the standpoint of nomenclature.

What did Neumann actually do? He reported, wholly without diagnosis, certain ticks from Texas and New Mexico which he found labelled *D. venustus* by Marx. In his opinion these specimens belonged to the old species *D. reticulatus*. It is impossible (as Horváth states in his dissenting opinion) to regard this as a valid publication. The name *venustus* was not applied as a substitute for *reticulatus,* or to any part of the genuine *reticulatus,* but to undescribed specimens associated by the author with that species. The position is better understood if we suppose the matter to still stand where it stood then. There would be no possibility of knowing anything definite about the so-called *D. venustus,* except the localities. A name which can not be understood or interpreted on the basis of what has been published is not validly published in the sense of Article 25 of the Code. On the other hand, it is questionable whether private type designation ought to overrule the action of a subsequent reviser of a composite "species." This is a matter of great importance, which ought to be dealt with in an authoritative manner. The following position is at least worthy of consideration:

(1) The standing of a species depends on the statements published by the author. If we admit the *obligation* to be governed by his unpublished actions, we tie the hands of subsequent workers, who can not know what he may or may not have done in manuscript.

(2) If the supposed species is composite, the first reviser who detects the fact is compelled to select part of the aggregate to carry the name, and this should stand if there is no distinct indication to the contrary in the original account.

(3) Whether the supposed species is composite or not, if several localities are cited, but no type locality, the first reviser should have the right to select a type locality from among those originally given.

Hence, unless the *description* of Banks distinctly contradicts it (and no one appears to assert that it does), although the valid name *D. venustus* dates from Banks, 1908, Stiles in 1910 was at liberty to restrict the name to that part of the composite represented by the material on sheep from Texas, this having been definitely cited by Banks.

T. D. A. COCKERELL
UNIVERSITY OF COLORADO

ACQUIRED CHARACTERS

ON January 2, 1923, Wm. J. Herdman, of Toronto, addressed the following letter to Johns Hopkins University:

> During the war, England, Canada and the United States gave their soldiers more than a year of intensive physical training before sending them to the front. The medical men were much interested in the effects produced by this training, and it is reported that the American Medical Association recommended universal military training for all men for the mental and physical benefits obtained thereby.
>
> Has there been any scientific research on man or the lower animals to determine whether such benefits are inherited by offspring?

This letter of inquiry also contained some further explanations to distinguish between environment and animal activities. The reply was written by Professor H. S. Jennings, and in it he referred to Kammerer's work on "the effects of changed environments" and said: "He reports inheritance of these environmental effects, throughout." Professor Jennings also referred to my books and articles, the experiments of Guyer, and a brief article in SCIENCE for December 15, 1922, by Griffith. Detlefsen was not mentioned.

To this Herdman replied:

> Since writing my letter of January 2d I have secured a copy of Redfield's "Human Heredity" (1921). As far as I have been able to discover, this, and his previous books which you mention, represent the only work bearing upon the question which I asked in my previous letter. I have not examined the German reference which you mention, but if those experiments relate to actions of the environment, as you say, then they are not what I was looking for. I was looking for results produced by actions of the animal, and not those produced by actions of the environment.

After giving some explanations of why the work of Guyer and Griffith did not answer the question asked, Herdman said:

> Redfield seems to be the only person who has furnished any information bearing on the effects of physical training continued for enough years to amount to anything, yet, from what you say, he seems to be under a cloud. . . . If there is only one source of information on a particular

subject, and that source is unreliable, then we have a queer situation. . . .

Professor Jennings waited two months and then wrote: "I felt that I had little or nothing to add that would help you, so that I have not hurried about replying."

The words "nothing to add" are a categorical acknowledgment that on March 14, 1923, Professor Jennings did not know of a thing on the face of the earth, other than my work, which was a proper answer to the question asked. When I gave out my quotations, I said that they were "simply those parts of the letters received which answer the questions asked—irrelevant matter being omitted."

The same letter of inquiry was also sent to the National Research Council, the Bussey Institution and to several other places. None of the other replies referred to Kammerer's work, or indicated any other person who had investigated the inheritance of "mental and physical benefits" obtained by training. As Professor Conklin put it: "I am sorry to have to tell you that there has been no satisfactory research of this character." No matter how unsatisfactory my work may be to any one, he can not deny that it is directed to the particular thing asked about in the question. Under these conditions I do not see any reasonable ground for the complaint of Professor Jennings in SCIENCE for January 11, 1924. I gave everything which was relevant, and the form of giving it did not change the meaning in the slightest degree.

CASPER L. REDFIELD

CHICAGO, ILL.

SINCE sending in my note in SCIENCE of January 11, the material there discussed has been published by Redfield. With relation to the above communication, two matters of fact require to be made clear:

(1) My letter to Herdman cited the paper (by Detlefsen) given at the scientific meetings in Toronto, but did not mention the author's name.

(2) Any one competent to discuss the inheritance of acquired characters knows that the experiments of Kammerer do deal with those effects of the environment that take the form of responses by the organism, including activities as well as structures. The same is true of the book by Semon, which was likewise cited.

The method employed in asserting that the words "nothing to add" are "a categorical acknowledgment" of what Redfield affirms, is a precious sample of the illuminating methods referred to in my former communication. One is at liberty to hold any opinions that suit his fancy, but to publish them over the signature of another who considers them preposterous is not scientific. The matter is without interest save as a study in the methods, reliability and competence of a man whose pronouncements on a difficult biological problem have in certain otherwise well-informed quarters been taken seriously.

H. S. JENNINGS

THE PROBLEM OF THE MONKEY AND THE WEIGHT

IN this *Journal* on February 15 last, page 164, Carl Hering states the problem in these words: "A supposedly weightless rope passing over a frictionless pulley has a 10 pound weight hanging on one end and a 10 pound monkey on the other. What will happen when the monkey climbs the rope?

As the proposer himself does not give a solution of his problem, it may be of interest to mention the practical test to which the writer put it a little over six years ago. A clockwork monkey driven by a spring and weighing 240 grams was counterpoised over the nine-inch pulley of a fine Atwood's machine. When the thread holding the last wheel in the train was burned, it climbed 80 cm in a minute, while the counterpoise remained stationary. An account of this was published in *School Science and Mathematics* in December, 1917, xvii, 821.

This statement remained unchallenged for two years, when in the same journal in December, 1919, xix, 815, Wilbert A. Stevens asserted that friction was to blame for the fact that the counterpoise did not rise with the monkey. This appeared rather doubtful because one fifth of a gram was sufficient to destroy the equilibrium, and when the pulley was replaced by a balance, 10 milligrams deflected the beam when the monkey and its counterpoise were attached. When the monkey started to climb, the counterpoise did go up for a moment, but it came down again and oscillated about its zero position with decreasing amplitudes.

The experiment was then repeated with the clockwork monkey climbing ten times as fast as before, and then both monkey and counterpoise went up together. This result was published in the same journal in February, 1920, xx, 172.

WILLIAM F. RIGGE

IN the discussion of this problem on page 164 of this volume of SCIENCE, a necessary condition has been omitted from the statement of the problem; that is, the relative distances of the monkey and the weight from the pulley.

Whatever this ratio may be, the effect of the action of the monkey in passing the rope through his hands must be precisely the same as that which would be produced by a shrinkage of the rope. That is, the tension will be the same in all parts of the rope. If the weight is five feet and the monkey ten feet from

the pulley at the start, the weight will reach the pulley while the monkey is still five feet from it. If both parties are ten feet from the pulley at the start, the monkey, when he reaches the pulley, will have passed twenty feet of rope through his hands, under a tension of ten pounds. That is, he will have expended twice the energy that would have been required, if the weight had been held motionless. As regards the influence of the inertia of the moving body (touched upon at the end of the article) it is evident that the energy expended at the beginning of the ascent to acquire velocity will be restored at the end of the trip as the velocity is gradually reduced to zero.

LAUNCELOT ANDREWS
WILLIAMSTOWN, MASS.

THIS problem to which Dr. Hering has called attention in SCIENCE, February 15, was invented by Lewis Carroll (C. L. Dodgson) in 1893. In the *American Mathematical Monthly*, volume 28, 1921, pp. 399–402, may be found a history of the problem, and solutions by Professors E. V. Huntington, of Harvard University, and L. M. Hoskins, of Stanford University. Neglecting also the weight of the pulley, the monkey and weight will move upwards at the same rate.

R. C. ARCHIBALD
BROWN UNIVERSITY

SCIENTIFIC BOOKS

The Conservation of Wild Life in Canada. By DR. C. GORDON HEWITT, Dominion Entomologist and Consulting Zoologist. Charles Scribner's Sons, New York, pp. 1–344, 1921.

THE late Dr. C. Gordon Hewitt's volume on Canadian wild life gives us for the first time a thoroughly comprehensive account of the larger mammals and birds of Canada from the conservation point of view. A distinctive feature of the book is its clear recognition of the fact that in Canada the wild life problem is, economically considered, largely a question of the proper utilization of the non-agricultural forest lands. He says:

> Not all lands are suitable for agriculture. Even in the best agricultural sections of the country areas unsuited to agriculture occur. In some cases, as will be shown later, such areas have been set apart as forest reserves; in other cases a struggling population endeavors to eke out a meager existence on the sparse products of the unfertile soil. The problem of the best method of dealing with such non-agricultural lands has already received some consideration by our governments. In the future it will demand more attention than we have hitherto thought necessary. And it is here primarily that the practical application of the principles of wild-life conservation should receive serious attention, for it will afford one of the most important methods by which the unproductive or scarcely productive areas can be rendered productive.

Northern Canada is so barren, in spite of Stefánsson to the contrary, that only detailed and extensive investigations will provide plans for a proper use of its resources. Dr. Hewitt remarks:

> The economic development of northern Canada is dependent upon the proper conservation of the wild life of that section of the Dominion. If adequate measures are adopted to conserve upon proper lines the game and fur-bearing animals of those portions of the Northwest Territories unsuited to agriculture, and such portions constitute by far the greater part, there is no reason why the whole of that area should not be productive and contribute to the wealth of the country. The possession of such territories would become a matter of pride rather than of reproach. . . . This country contains the greatest variety of valuable fur-bearing animals, for the possession of which men risked everything, including their own lives. Now our agricultural lands constitute that lure, but the remnant of those fur-bearing animals is still with us. Conservation of our natural resources is taking the place of exploitation. We should apply the doctrine to the fur-bearing animals and thus secure their full value to the community. And it can not be stated too often that conservation means the protection of natural resources from injudicious exploitation and their provident utilization. Our northern territories, under proper administration, could become not only a valuable source of food supply, but also one of the chief fur-producing areas of the world.

The chapter devoted to the enemies of wild life and the method of controlling predatory animals contains much interesting data on Canadian conditions and gives considerable additional evidence of the failure of the bounty system as a satisfactory method of controlling these animals. Hewitt states:

> And while the complete extermination of such predatory species is not possible, desirable, or necessary, a degree of control must be exercised to prevent such an increase in numbers as would affect the abundance of the non-predatory species.

One of the most valuable parts of the book is the chapter on the periodic fluctuation in the number of fur-bearing animals. He furnishes graphs showing the fur returns of the Hudson's Bay Company from 1821 to 1914 and thus summarizes his conclusions:

> First, the herbivorous rodents such as mice and rabbits, which are very prolific and increase in numbers until they reach an abundance which causes overcrowding, when an epidemic of disease almost wipes them out and their numbers rapidly decrease to a minimum. Second, we have the numerous predatory animals which depend for their subsistence either directly or indirectly upon the mice and rabbits. These animals exhibit fairly regular periodic fluctuations in numbers, their abundance being

correlated with the abundance of the animals upon which they feed, although, as we pointed out in the case of the fisher, there may be a distinct periodic fluctuation which does not appear to be directly related to the fluctuation in the numbers of any particular food animal. Finally, we have the animals that feed on a mixed or exclusive diet of insects, vegetable products, fish, or miscellaneous diet, that do not show any marked periodic fluctuations. (P. 232.)

These periodic phenomena should be carefully compared with other periodic activities, such as that of rainfall, sun spots and periodic tree growth, as investigated by Huntington and Douglass.

Just as in the United States the management of fish and game has been largely in the hands of the states, so in Canada it has been largely in the hands of the provinces. But in the unorganized Northwest Territories and the Yukon Territory the Dominion government controls, as also in the case of the treaty with the United States with regard to migratory birds, in the execution of which Hewitt played an important rôle. At his suggestion the Northwest Game Act has been changed to give adequate protection for the fur-bearing animals, by a license system, and to give as well better protection to the musk ox and the barren-ground caribou. Hewitt points out that the fur trade was the primary industry of the region, and that unless properly maintained this enormous area of the Dominion would become incapable of supporting a population, and that its natives would starve or become a burden on the government. He also points out that the Danish government maintains a monopoly of the fur trade in Greenland, and in this manner controls trapping in such a way as to conserve the breeding stock. By applying this method of a "sustained yield" to fur, to borrow a forester's term, a stabilized and permanent industry would be guaranteed. On account of the paramount interest of the Dominion government in the native population, Dr. Hewitt suggests:

> A careful consideration of the problem of our northern fur resources and the position of the native population in relation to the exploitation of such resources serves to impress one with the fact that the taking over and administration by the Dominion Government of the fur trade of the Northwest Territories would be most desirable from all points of view. The following proposal is therefore made. The Dominion Government should take over the entire control and exploitation of the fur trade and wild life resources of the Northwest Territories by enacting the necessary legislation. This would involve the purchase of such rights as the Hudson's Bay Company have in the Northwest Territories. In order to administer the monopoly it would be necessary to establish certain government posts such as those now maintained by the Hudson's Bay Company. This would be a great advantage in securing proper and adequate government administration in the Northwest Territories, where the need of government agents to take charge of the affairs of the Indians, the enforcement of the law, the collection of customs, and oversight of other government activities in these territories is becoming increasingly felt, and will undoubtedly become greater with their development. . . .
>
> If such a policy were adopted it would accomplish the following ends. A source of revenue would be created of no small value, even after the expenses of administration were paid, and it is proper that the profits accruing from the exploitation of the fur resources of these territories should go into the national exchequer. It would be the most effective method of conserving the fur resources and wild life of the Northwest Territories, as the enforcement of the law and the adoption of any necessary restrictive measures could be directly supervised. It would afford a means of attending to the requirements of the natives who stand in need of more immediate supervision, which is difficult to give at the present time. The natives, both Indian and Eskimo, would be protected to a greater degree than at present from the influence and exploitation of unscrupulous traders, which would be an advantage from the standpoint of morals and health.
>
> The nationalization of such natural resources as forests has proved in Europe to be the most successful means of conserving such resources, and at the same time this policy has furnished a valuable source of national revenue. There is no good reason why such a policy should not be adopted in the case of our northern fur resources. The thoughtful consideration of this purpose is therefore respectfully urged. (Pp. 261–262.)

There is still another important Dominion control exerted over wild life, and that is in the Canadian National Parks. One of the main aims for these parks is for our own generation to pass on to the future the wild life of these regions. The intelligent formulation of satisfactory rules for the protection of wild life and their enforcement, is a subject that should receive the most careful attention of Canadian naturalists, until the Canadian Parks are properly staffed with scientific men, clothed with authority as well as with responsibility. Naturalists are only beginning to awaken to the importance and seriousness of this matter. There is an urgent, acute need for careful, scientific study of the predatory animals in these parks (and elsewhere for that matter), because of the prevalence of a strong prejudice against the predatory animal. A sane, long look ahead is what is needed in this matter. It is only too easy to order out the trapper or hunter to kill these animals, without a previous and adequate study of the whole situation. The eagerness with which the uninformed public devours news items concerning the destruction of supposedly dangerous animals readily makes fuel for cheap politicians.

The general public will probably not miss the absence of a bibliography such as the author must have acquired during the preparation of the volume,

but it would have added considerably to the value of the book for those who are unfamiliar with the local sources of information. One also regrets that Labrador and Newfoundland were not included within its field, because our American public needs guidance there as well as elsewhere in British America.

Considering the book as a whole, it must be considered a very valuable summary and contribution to the literature on American wild life conservation. The sane and judicial method of presentation carries with it a confidence that will do much for the cause. We regret the loss of such an able leader as Dr. Hewitt and hope that a new one will be found to continue this excellent work.

CHARLES C. ADAMS

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LABORATORY APPARATUS AND METHODS

THE LABORATORY AND DEMONSTRATION PROBLEM OF MODERN PHYSICS

SOME of the more recently developed fields of physics, constituting what some writers call "modern physics," appear to offer some problems relative to laboratory practice more or less peculiar to the subject-matter involved and distinct from those of general courses. Certainly, the content and methods of these courses show far greater variation among the universities than the older courses which have become more or less standardized. In view of this it appeared worth while to ascertain by means of a questionnaire sent out to a number of the larger universities just what is the trend of present practices along this line. Information from about twenty-five laboratories was thus secured, and last year the material thus obtained was supplemented by personal visits to a few laboratories and through contact with physicists at meetings.

It would appear that, so far as lecture work along these lines is concerned, practically every university is giving definite attention to the newer fields of the subject of physics. A few merely include a number of lectures along such lines in their general courses. Many offer separate courses which often include what might be called the whole field, under some such title as "Modern physics," "Electron theory," etc., and deal with the subject in a very generalized way, but nevertheless in a way designed to meet the needs and interests of the arts students. The larger universities, however, are now offering a number of more or less clearly differentiated courses in this general field, some being offered yearly but many less frequently and depending upon the demand. Such schools usually have one or two courses of the above mentioned general type which are open to the undergraduate student. Their other courses are more or less mathematical in nature and are open to only those advanced undergraduates or graduates specializing in physics. Physics club meetings frequently serve to bridge the gap between these two groups of students and to foster an interest in the present progress of the science. In the general modern physics course for undergraduates the enrollment is usually good, one university reporting a class of seventy, but the number enrolling in the advanced courses is, as might be expected, usually small.

The laboratory practice, however, in connection with such courses is far less uniform, both in respect to what is actually done, and what those offering the courses think should be done. Of the schools reporting courses along these lines slightly more than half reported laboratory work as a part of the course, and the time given to this work is usually one afternoon a week. In one case four or five laboratory experiments were given, but no regular laboratory period was scheduled. In a number of schools a few experiments along the lines here discussed are worked into other courses. In schools offering no individual work, a number of lecture demonstration experiments are given.

The nature of the laboratory work varies widely, and in a manner dependent upon the training and dominant interests of the instructor. Experiments intended for undergraduates are somewhat qualitative and no doubt may continue so. For more advanced students the work frequently consists in repeating some of the more classical experiments, or in making observations with apparatus previously assembled by research students. In only a few cases were typed instruction sheets submitted to illustrate the nature of the work done; generally it was indicated that the work was as yet merely in the formative stage. In spite of this, however, it would appear that, through the personal exchange of ideas and the publication of manuals, experiments along these lines are rapidly working towards something like a uniformity that will make a student's laboratory course in modern physics mean something definite in the way of his training.

The subject-matter of the experiments reported would indicate that not all the chief phases of the present developments lend themselves easily to laboratory studies by the student, although the gradual elimination of the difficulties must widen the scope of the work attempted. Among the studies attempted, the following partial list will serve to indicate the scope and nature of the work:

"Discharge in air at low pressures," "Variation of force with distance between electrodes," "Study of Braun tube (in modified form)," "Cathode rays (including e/m determinations)," "Canal rays and Dop-

pler effects," "Studies of quadrant electrometers," "Calibrations of electroscopes," "Ionization currents," "Properties of ionized gases," "Mobility of ions," "Formation of clouds by expansions," "Conductivity of flames," "Comparison of ionizing agents," "Thermionic currents," "Studies with three-electrode vacuum tubes," "Photo-electric currents," "Brownian movements," "X-ray tubes," "X-ray photography," "X-ray spectra," "Ionizing potentials;" and a group of experiments in the field of radio-activity such as "Range of Alpha particles and stopping power of aluminum by scintillation method," "Absorption of Beta particles," "Absorption of Gamma rays," "Rate of decay of thorium emanation," "Determination of e/m for Beta rays by magnetic deflection," and others similar to those suggested in Makower and Geiger's book or in Crowther's.

In some instances additional work is given along the line of glass-blowing and other physical manipulations and in rendering assistance to research students.

Considerable doubt seems to exist as to the advisability of offering, particularly to undergraduate students, laboratory work in courses along the lines here discussed; in fact, all recognize the inherent difficulties. Certainly, the laboratory work does not at present lend itself to the routine methods commonly employed in other courses nor to the supervision of student assistants. More than one laboratory period for a single experiment is usually required—partly on account of the special technique involved. The nature of the apparatus is such that duplicate sets are hardly to be expected in most cases, and a mistake by one group of students may upset the program of experiments and involve a replacement of expensive or difficultly made apparatus. To guard against such occurrences the instructor must personally give more time to supervision than his time may well permit. In view of all this one may easily understand the following quotations from the replies received:

We have tried them on our graduate students as laboratory experiments and find they require entirely too much time for the returns.

In my opinion the subjects are not suitable for elementary laboratory work.

My experience so far with the laboratory work has not been encouraging, I hope to arrive at a solution of the difficulty this term.

This laboratory work is very satisfactory but requires much time on the part of the student and the teacher, nevertheless it is a very helpful course.

However, most of the replies indicate great interest in the problem and, like the last quoted, favor the laboratory work where time and equipment permit in spite of the difficulties.

I think the course amply justifies the slight additional labor of looking after apparatus, because of the growing importance of a knowledge of . . . .

We find the laboratory work satisfactory as far as it goes, but our time is entirely too limited.

I am in favor of it . . . .

The writer of the latter statement strongly advocates permanent set-ups for some of the more important recent experimental contributions to modern physics.

The present writer firmly believes in giving due emphasis to the experimental phases of modern physics. Many of the problems lend themselves to student laboratory work, and the problem of offering such work will grow easier as apparatus companies develop new and simplified equipment and as manuals descriptive of experiments are published. Even where time and facilities do not permit individual laboratory work the need may be largely met by giving greater emphasis to the lecture demonstration. Too often it is assumed that after a student has finished a few general courses in physics he has no further need of experimental evidence, as he is then able to visualize clearly everything from the printed page. A common practice is to run through in a single day in a general course a "whole show" of electron phenomena and to imagine the student has followed the whole demonstration intelligently, and therefore needs little more of apparatus in later courses. May it be suggested, then, that the student in these special courses may find the field just as new to him and probably further removed from his experience as his general course was to him, and consequently demonstration and laboratory work, appropriately selected and abbreviated, may easily be just as important, from both the instructional and inspirational standpoints, and function in the same way as in his general courses. A more advanced student may be better equipped, but his mental processes are not greatly changed.

Basing his judgment on his own experience, first as a student and later as an instructor, the present writer is quite convinced that more attention to this question might contribute to even greater interest on the part of the student in the recently developed and fascinating fields of physics.

E. L. HARRINGTON

UNIVERSITY OF SASKATCHEWAN

ENDO AGAR AS AFFECTED BY PEPTONE

FOR several years considerable difficulty has been experienced in bacteriological laboratories in making Endo agar. The plates, after being poured with the sterile, decolorized medium and then inoculated with organisms of the colon group, would become, after incubation, a deep pink color which diffused over the entire plate and, at least partially, masks the characteristic reaction of the organisms being studied. This trouble has been attributed to various causes such as

unstable fuchsin, changes in reaction of the agar due to heating or impurity in the chemicals used, etc.

In an experiment in which testing of various stains was the object, the writer used two different lots of Endo agar; one of which was made up with an American peptone, the other with Witte peptone. In all other respects the two lots were identically the same. The formula used in making up the Endo agar was as follows:

5 grams of Liebig's beef extract.
10 grams of peptone.
30 grams of dehydrated agar.
1000 cc of distilled water.

This medium was adjusted to pH 7.0 by Clark's colorimetric method, divided into 100 cc portions and autoclaved at 15 lbs. pressure for 15 minutes. When ready for use the following materials were added to 100 cc of agar: 15 cc of water in which 1 gram of lactose had been dissolved and 10 cc of a 2.5 per cent. sodium sulphite solution in which .5 cc of a 1 to 10 alcoholic solution of basic fuchsin had been decolorized. Plates were poured with this medium and incubated for 24 hours before inoculating to test their sterility.

When these uninoculated plates were examined after incubation a surprising difference in appearance was noted. The sterile plates made from the Witte peptone were colorless while those made from the American peptone were a deep rose pink completely and uniformly diffused over the entire plate. Upon inoculation of duplicate plates of the different media with *Escherichia coli* and *Aerobacter aerogenes* the results were still more striking. After 24 hours' incubation at 37 degrees C. the plates of Witte peptone agar showed a characteristic growth of these organisms. The medium was colorless, except around area of growth where the usual reaction appeared. Those made from the American peptone were a deep, dark red diffused over the entire plate with a darker area in the region of growth. The characteristic coloring of the two organisms was masked by the deep color of the agar.

This work was immediately checked by a duplicate experiment. Two new lots of Endo agar were again made up using the same formula and the two different peptones. Each lot was divided and adjusted to two different reactions, one a distinctly alkaline, and the other neutral. Clark's colorimetric method was used. The same fuchsin was used in both experiments. The results were the same. No difference could be seen in the plates due to difference in reaction.

Sterile plates of each lot were left exposed to the light and air several days. In case of the American peptone plates the color diffused over the plates in 48 hours, while at the end of four days only a faint pink was noted in the Witte peptone plates.

These results indicate that the diffusion of color in Endo agar plates may be influenced by the kind of peptone used. Further experiments are in progress which involve the use of various different kinds of peptone as well as different kinds of fuchsin. These will be reported as soon as they are completed.

ELIZABETH F. GENUNG
SMITH COLLEGE

THE AMERICAN PHYSIOLOGICAL SOCIETY

THE thirty-eighth annual meeting of the American Physiological Society was held December 27 and 28, at the Washington University School of Medicine, and December 29, 1923, at the St. Louis University School of Medicine. The meeting was unusually well attended from the central territory and had a fair attendance from the eastern seaboard.

At the annual business meeting the following were the chief events voted by the society:

Announcement was made of the continuation of the Wm. T. Porter Fellowship for physiological research, administered under the auspices of the American Physiological Society. Dr. Florence B. Seibert, Ph.D, of Yale, was appointed research fellow for the second time. Dr. Seibert elected to pursue her research work in the laboratories of Dr. H. Gideon Wells of the University of Chicago.

Report by President A. J. Carlson, the representative of the society on the National Research Council, covered the matter of medical fellowships and called attention to the fact that a number of these fellows had elected to further their training and research in the physiological group.

The representatives of the society on the Council of the Union of American Biological Societies, President A. J. Carlson and C. W. Greene, reported the movement perfecting the plan for publication of a comprehensive Biological Abstracts. Under the chairmanship of Dr. J. R. Schramm, of Cornell University, the details for this publication are now approaching the point where the undertaking will be launched. The union is also fostering the establishment of a tropical biological station, and is assisting in the formation and work of an international committee on nomenclature.

The invitation extended to the International Physiological Congress for the meeting in America in 1925 was placed with the International Committee by the president of the society at the Edinburgh meeting last July. The final decisions, however, have not been made.

Dr. D. R. Hooker was elected managing editor of the *American Journal of Physiology* for the year 1924. The council nominated as the editorial board of